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**XV. Experiments and Observations relating to Air and Water.**  
*By the Rev. Joseph Priestley, LL.D. F.R.S.*

Read February 24, 1785.

**E**VER since the discovery of the diminution of respirable air in those processes which are generally called *phlogistic*, it has been a great object with philosophers to find what becomes of the air which disappears in them. Among others, I have made and published a variety of experiments with that view; but though by this means some farther progress was made in the philosophy of air, and consequently our knowledge of the principles, or constituent parts, of natural substances was extended, I did not by any means succeed to my satisfaction with respect to the immediate object of my researches. Others, however, were more successful, and their success has at length enabled me to resume my experiments with more advantage; by which means I have been led both to confirm their conclusions, and, by diversifying the experiments, to throw considerable light upon various other chemical processes. The result of these observations I shall lay before the Society, with as much brevity and distinctness as I can.

In the experiments of which I shall now give an account, I was principally guided by a view to the opinions which have lately been advanced by Mr. CAVENDISH, Mr. WATT, and M. LAVOISIER. Mr. CAVENDISH was of opinion, that when *air* is decomposed, *water* only is produced; and Mr. WATT concluded

concluded from some experiments, of which I gave an account to the Society, and also from some observations of his own, that water consists of dephlogisticated and inflammable air, in which Mr. CAVENDISH and M. LAVOISIER concur with him; but Mr. LAVOISIER is well known to maintain, that there is no such thing as what has been called *phlogiston*, affirming inflammable air to be nothing else but one of the elements or constituent parts of water. In the following experiments I also had a particular view to a conclusion which I had drawn from those experiments, of which an account is given in my last communications to the Royal Society; *viz.* that inflammable air is pure phlogiston in the form of air, at least with the element of *heat*; and that fixed air consists of dephlogisticated and inflammable air; both which doctrines had been first advanced by Mr. KIRWAN, before I had made the experiments which I then thought clearly proved them.

Such were the hypotheses to which I had a view when I began the following course of experiments, which I hope will be an admonition to myself, as well as to others, to adhere as rigorously as possible to *actual observations*, and to be extremely careful not to overlook any circumstance that may possibly contribute to any particular result. I shall have occasion to notice my own mistakes with respect to *conclusions*, though all the *facts* were strictly as I have represented them. But whilst philosophers are faithful narrators of what they observe, no person can justly complain of being misled by them; for to *reason* from the facts with which they are supplied is no more the province of the person who discovers them, than of him to whom they are discovered.

One of the most simple of all phlogistic processes is that in which metals are ignited in dephlogisticated air. I therefore

began with this, with a view to ascertain whether any *water* is produced when the air is made to disappear in it. Accordingly, into a glass vessel containing 7 ounce measures of pretty pure dephlogisticated air, I introduced a quantity of iron turnings (which is iron in small thin pieces, exceedingly convenient for these and many other experiments) having previously made them, together with the vessel, the air, and the mercury by which it was confined, as dry as I possibly could. Also, to prevent the air from imbibing any moisture, I received it immediately in the vessel in which the experiment was made, from the process of procuring it from red precipitate; so that it had never been in contact with any water.

I then fired the iron, by means of a burning lens, and presently reduced the 7 ounce measures of air to .65; but I found no more water after this process than I imagined it had not been possible for me to exclude, as it bore no proportion to the air which had disappeared. Examining the residuum of the air, I found one-fifth of it to be fixed air, and when I tried the purity of that which remained by the test of nitrous air, it did not appear that any phlogisticated air had been produced in the process: for though it was more impure than I suppose the air with which I began the experiment must have been, it was not more so than the phlogisticated air of the 7 ounce measures, which had not been affected by the process, and which must have been contained in the residuum, would necessarily make it. In this case one measure of this residuum and two of nitrous air occupied the space of .32.

In another experiment of this kind, ten ounce measures of dephlogisticated air were reduced to .8, and by washing in lime water to .38. In another experiment, in which  $7\frac{1}{2}$  ounce measures of dephlogisticated air were reduced to half an ounce

measure, of which one-fifth was fixed air, the residuum was quite as pure as the air with which I began the experiment, the test with nitrous air, in the proportions above-mentioned, giving .4 in both cases. To what circumstance the difference might be owing I cannot tell.

In these experiments the fixed air must, I presume, have been formed by the union of the phlogiston from the iron and the dephlogisticated air in which it was ignited; but the quantity of it was very small in proportion to the air which had disappeared, and at that time I had no suspicion that the iron, which had been melted, and gathered into round balls, could have imbibed it; a melting heat having been sufficient, as I had imagined, to expel every thing that was capable of assuming the form of air from any substance whatever. I was therefore intirely at a loss about what must have become of the air.

Sensible, however, that such a quantity of air must have been imbibed by *something* to which it must have given a very perceivable addition of weight, and seeing nothing else that could have imbibed it, it occurred to me to weigh the calx into which the iron had been reduced; and I presently found, that the dephlogisticated air had actually been imbibed by the melted iron, in the same manner as inflammable air, in my former experiments, had been imbibed by the melted calces of metals, however impossible such an absorption might have appeared to me *a priori*. In the first instance, about twelve ounce measures of dephlogisticated air had disappeared, and the iron had gained six grains in weight. Repeating the experiment very frequently, I always found, that other quantities of iron, treated in the same manner, gained similar additions of weight, which was always very nearly that of the air which had disappeared.

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This calx of iron, I then concluded, was by no means what I had before taken it to be, *viz.* a *pure calx* or *slag*, but either the calx, or the iron itself, saturated with pure air. This calciform substance I found, by various experiments, to be the same thing with the *scales* that fly from iron when it is made red-hot, or the substance into which it runs in a very intense heat, in an open fire.

Concluding from the preceding experiment, that iron, sufficiently heated, was capable of saturating itself with pure air, extracted from the mass of the atmosphere, I then proceeded to melt it with the heat of a burning lens in the open air; and I presently found, that perfect iron was easily fused in this way, and continued in this fusion a certain time, exhibiting the appearance of *boiling* or *throwing out* air, whereas it was on the contrary *imbibing* air; and when it was saturated the fusion ceased, and the heat of my lens could not make any farther impression upon it. When this was the case, I always found that it had gained weight in the proportion of  $7\frac{1}{2}$  to 24, which is very nearly *one-third* of its original weight. The same was the effect when I melted *steel* in the same circumstances, and also every kind of iron on which the experiment could be tried. But I have some reason to think, that with a greater degree of heat than I could apply, the iron might have been kept in a state of fusion somewhat longer, and by that means have imbibed more air, even more than one-third of its original weight.

There was a peculiar circumstance attending the melting of *cast iron* with a burning lens, which made it impossible to ascertain the addition that was made to its weight, and at the same time afforded an amazing spectacle; for the moment that any quantity of it was melted, and gathered into a round ball, it began

to disperse in a thousand directions, exhibiting the appearance of a most beautiful fire-work, some of the particles flying to the distance of half a yard from the place of fusion; and the whole was attended with a considerable hissing noise. Some of the largest pieces which had been dispersed in this manner I was able to collect, and having subjected them to the heat of the lens, they exhibited the same appearance as the larger mass from which they had been scattered.

When I melted this cast iron in the bottom of a deep glass receiver, in order to collect all the particles that were dispersed, they firmly adhered to the glass, melting it superficially, though without making it crack, so that it was still impossible to collect and weigh the particles. However, I generally found that, notwithstanding the copious dispersion, what remained after the experiment rather exceeded than fell short of the original weight of the iron. Sometimes a piece of common iron, and especially steel, would make a little hissing in the fusion, and a particle or two would fly off; but this was never considerable\*.

Having now procured what I thought to be a new calx of iron, or a calx saturated with pure air, I endeavoured to revive it by making it imbibe inflammable air, in the same manner that I had before made iron, and various other metals, by melting them in a vessel containing inflammable air. In this I succeeded; but in the course of the experiment a new and very unexpected appearance occurred. I took a piece of iron which I had saturated with pure air, and putting it into a glass vessel

\* On being informed of the above-mentioned phenomena, Mr. WATT concluded, that the basis of the dephlogisticated air united to the phlogiston of the iron, and formed *water*, which was attracted by, and remained so firmly united to the calx of iron, as to resist the effects of heat to separate them.

containing inflammable air, confined by water, threw upon it the focus of the lens, and presently perceived the inflammable air to disappear, and without thinking of any thing escaping from the calx of iron (which had been subjected to a greater heat before) I imagined that I should have found the addition of the weight of air in the iron, and the result might be an iron different from the common sort. But I found, to my surprise, that the iron, which had exhibited no new appearance in this mode of treatment, had lost weight, instead of gaining any. The piece of iron on which I made this first experiment weighed  $11\frac{1}{2}$  grains, and  $7\frac{1}{2}$  ounce measures of inflammable air had disappeared while the iron had lost  $2\frac{1}{2}$  grains.

Considering the quantity of inflammable air that had disappeared, viz.  $7\frac{1}{2}$  ounce measures, and the dephlogisticated air which had been expelled from the iron, viz.  $2\frac{1}{2}$  grains, which is equal to about 4.1 ounce measures, I found that they were very nearly in the proper proportion to saturate each other, when decomposed by the electrical spark, viz. two measures of inflammable air to one of dephlogisticated air. I therefore had now no doubt but that the two kinds of air had united, and had formed either *fixed air* or *water*; but which it was I could not tell, having had water on the receiver in which the experiment was made, and having neglected to examine the state of the air that remained, except in a general way, by which I found, that it was still, to appearance, as inflammable as ever.

With a view to determine whether *fixed air*, or *water*, would be the produce of this mode of combining inflammable and dephlogisticated air, I repeated the experiment in a vessel in which the inflammable air was confined by mercury, and both the vessel and the mercury had been previously made as dry as possible. I had no sooner begun to heat the iron, or rather *slag*,



in these circumstances, than I perceived the air to diminish, and at the same time the inside of the vessel to grow very cloudy, with particles of dew, that covered almost the whole of it. These particles by degrees gathered into drops, and ran down the sides of the vessel in all places, except where it was heated by the sun-beams; so that it then appeared to me very evident, that *water*, with or without fixed air, was the produce of the inflammable air, and the pure air let loose from the iron in this mode of operation; though afterwards I was taught by Mr. WARR to correct this hypothesis, and to account for this result in a different manner. When I had examined the remaining air, it was as inflammable as ever, without containing any mixture of fixed air at all.

When I collected the water which was produced in this experiment by means of a piece of filtering paper, carefully introduced to absorb it, I found it to be, as nearly as possible, of the same weight with that which had been lost by the iron: and also, in every experiment of this kind, in which I attended to this circumstance, I found that the quantity of inflammable air which had disappeared was about double to that of the dephlogisticated air set loose from the iron, supposing that weight to have been reduced into air. Thus at one time I made a piece of this flag imbibe  $5\frac{1}{2}$  ounce measures of inflammable air, while it lost as much as the weight of about 3 ounce measures of dephlogisticated air, and the water collected weighed 2 grains. Another time a piece of flag lost 1.5 grains, and the water produced was 1.7 grains; but perfect accuracy is not to be expected. I shall only mention one more experiment of this kind, in which  $6\frac{1}{2}$  ounce measures of inflammable air were reduced to .92 ounce measure, and the iron had lost 2 grains, equal in weight to 3.3 ounce measures of dephlogisticated air.

In all the above-mentioned experiments, the inflammable air was that which is produced by the solution of iron in acids.

As before I had finished this course of experiments I had satisfied myself that inflammable air always contains a portion of water, and also, that when it has been some time confined by water, it imbibes more, so as to be increased in its specific gravity by that means, I repeated the experiment with inflammable air which had not been confined by water, but which was received in a vessel of dry mercury from the vessel in which it was generated; but I presently perceived that water was produced in this case also, and to appearance as copiously as in the former experiment. Indeed, the quantity of water produced, which so greatly exceeded the weight of all the inflammable air, is sufficient to prove that it must have had some other source than any constituent part of that air, or the whole of it, together with the water contained in it, without taking into consideration the corresponding loss of weight in the iron.

I must here observe, that the iron slag which I had treated in this manner, and which had thereby lost the weight which it had acquired by melting in dephlogisticated air, became *perfect iron* as at first, and was then capable of being melted by the burning lens again; so that the same piece of iron would serve for these experiments as long as the operator should chuse. It was evident, therefore, that if the iron had lost its phlogiston in the preceding fusion, it had acquired it again from the inflammable air which it had absorbed; and I do not see how the experiment can be accounted for in any other way, which necessarily implies the reality of phlogiston as a constituent principle in bodies. This, at least, is the most natural way of accounting for the appearances.

Having had this success with the calx, or scales of *iron*, I tried the calx of *copper*, or those scales which fly from it when it is made red-hot; and I found water produced in the inflammable air in the same manner as when I used the scales of iron in the same circumstances. I also had the same result when I revived *precipitate per se* in inflammable air; but having at that time a very weak winter's sun, I could not make the experiment with so much advantage as I could have wished.

Iron, I found, acquired this additional weight by melting in an earthen retort, as well as in the open air by the sunbeams, if it were possible for it to attract air, or whatever else it is that is the immediate cause of its additional weight. Three ounces of common iron filings, exposed to a strong heat in an earthen retort, gained 11 dwts, or 264 grains, and yet was very far from having been completely fused. Having a glass tube communicating with the retort, in order to collect any air that the iron filings might give out, I found that when they were very hot, the water ascended within the tube; which shews that the iron was then in a state of absorbing, and not of giving out any air.

Seeing so much water produced in these experiments with inflammable air, I was particularly led to reflect on the relation which they bore to each other, and especially to Mr. CAVENDISH's ideas on the subject. He had told me that, notwithstanding the experiments of which I had given an account to the Royal Society, and from which I had concluded that inflammable air was pure phlogiston, he was persuaded that *water* was essential to the production of it, and even entered into it as a constituent principle. At that time I did not perceive the force of the arguments which he stated to me, especially as, in the experiments with charcoal, I totally dispersed any quantity  
of

of it with a burning lens *in vacuo*, and thereby filled my receiver with nothing but inflammable air. I had no suspicion that the wet leather on which my receiver stood could have any influence in the case, while the piece of charcoal was subject to the intense heat of the lens, and placed several inches above the leather. I had also procured inflammable air from charcoal in a glazed earthen retort two whole days successively, in which it had given inflammable air without intermission. Also iron filings in a gun-barrel, and a gun-barrel itself, had always given inflammable air whenever I tried the experiment.

These circumstances, however, deceived me, and perhaps would have deceived any other person; for I did not know, and could not have believed, the powerful attraction that *charcoal*, or *iron*, appear to have for *water* when they are intensely hot. They will find, and attract it, in the midst of the hottest fire, and through any pores that may be left open in a retort; and iron filings are seldom so dry as not to have moisture enough adhering to them, capable of enabling them to give a considerable quantity of inflammable air. But my attention being now fully awake to the subject, I presently found that the circumstances above mentioned had actually misled me; I mean with respect to the *conclusion* which I drew from the experiments, and not with respect to the experiments themselves, every one of which, I doubt not, will be found to answer, whenever they are tried by persons of sufficient skill and properly attentive to all the circumstances.

Being thus apprised of the influence of unperceived moisture in the production of inflammable air, and willing to ascertain it to my perfect satisfaction, I began with filling a gun-barrel with iron filings in their common state, without taking any particular precaution to dry them, and I found that they gave air as

they had been used to do, and continued to do so many hours; I even got ten ounce measures of inflammable air from two ounces of iron filings in a coated glass retort. At length, however, the production of inflammable air from the gun-barrel ceased; but on putting water into it, the air was produced again, and a few repetitions of the experiment fully satisfied me that I had been too precipitate in concluding that inflammable air is pure phlogiston.

I then repeated the experiment with the charcoal, making the receiver, the stand on which I placed the charcoal, and the charcoal itself, as dry and as hot as possible, and using cement instead of a wet leather to exclude the air. In these circumstances I was not able, with the advantage of a good sun, and an excellent burning lens, to decompose quite so much as two grains of the piece of charcoal, which gave me ten ounce measures of inflammable air; and this, I imagine, was effected by means of so much moisture as was deposited from the air in its state of rarefaction, and before it could be drawn from the receiver. To the production of this kind of inflammable air I was therefore now convinced, that water is as necessary as to that from iron.

It was in this state of my experiments that I received an authentic account of those of M. LAVOISIER, on transmitting water through an hot iron tube and also through a hot copper tube containing charcoal, and thereby procuring large quantities of inflammable air, M. LAVOISIER himself having been so obliging as to send me a copy of his Memoir on that subject. I had heard an account of the experiments some months before; but it was so imperfect a one, that I own I paid little attention to them. At this time, however, I was prepared to be sufficiently sensible of their value.

In my last communications to the Royal Society, it will be seen that I had transmitted the vapour of several fluid substances through red-hot *earthen tubes*, and thereby procured different kinds of air. M. LAVOISIER adopted the same process, but used an *iron tube*; and by means of that circumstance made a very valuable discovery which had escaped me. I had indeed on one occasion made use of an iron tube, and transmitted steam through it; but not having at that time any view to the production of *air*, I did not collect it at all, contenting myself with observing that *water*, after being made red-hot, was still water, there being no change in its sensible properties. Being now farther instructed by the experiment of M. LAVOISIER, I was determined to repeat the process with all the attention I could give to it; but I should not have done this with so much advantage, if I had not had the assistance of Mr. WATT, who always thought that M. LAVOISIER's experiments by no means favoured the conclusion that he drew from them. As to myself, I was a long time of opinion that his conclusion was just, and that the inflammable air was really furnished by the water being decomposed in the process. But though I continued to be of this opinion for some time, the frequent repetition of the experiments, with the light which Mr. WATT's observations threw upon them, satisfied me at length that the inflammable air came principally from the charcoal, or the iron.

I shall first relate the result of the experiment that was made with *charcoal*, and then those with iron and other substances, in contact with which (when they were in a state of fusion, or at least red-hot) I made steam, or the vapour of other liquid substances, to pass. I shall only observe that, previous to this, I began to make the experiments with coated glass tubes, which

I found to answer very well during the process, though they never failed to break in cooling. At length I procured a tube of *copper*, on which, as M. LAVOISIER discovered, steam had no effect; and at last I made use of earthen tubes, with which Mr. WEDGEWOOD, that most generous promoter of science, liberally supplied me for the purpose; and these glazed on the outside only I find far preferable to copper. They are, indeed, every thing that I could wish for in experiments of this kind; the reason of which will appear in my account of another course of experiments, which I hope to lay before the Society in due time.

The disposition of the apparatus, with which these experiments were made, was as follows. The water was made to boil in a glass retort, which communicated with the copper or earthen tube which contained the charcoal or iron, &c. and which, being placed in an horizontal position, was surrounded with hot coals. The end of this tube opposite to the retort communicated with the pipe of a common *worm tub*, such as is generally used in distillations, by means of which all the superfluous steam was condensed, and collected in a proper receptacle, while the air which had been produced, and had come along with it through the worm tub, was transmitted into a trough of water, where proper vessels were placed to receive it, and ascertain the quantity of it; after which I could examine the quality of it at leisure.

In the experiment with *charcoal*, I found unexpected difficulties, and considerable variations in the result; the proportion between the *charcoal* and *water* expended, and also between each of them and the *air* produced, not being so nearly the same as I imagined they would have been. Also the quantity of fixed air that was mixed with the inflammable air varied very much.

much. This last circumstance, however, some of my experiments may serve to explain. Whenever I had no more water than was sufficient for the production of the air, there was never any sensible quantity of uncombined fixed air mixed with the inflammable air from charcoal. This was particularly the case when I produced the air by means of a burning lens in an exhausted receiver, and also in an earthen retort with the application of an intense heat. I therefore presume, that when the steam transmitted through the hot tube containing the charcoal was very copious, the fixed air in the produce was greater than it would otherwise have been. The extremes that I have observed in the proportion of the fixed to the inflammable air have been from one-twelfth to one-fifth of the whole. As I generally produced this air, the latter was the usual proportion; and this was exclusive of the fixed air that was intimately combined with the inflammable air, and which could not be separated from it except by decomposition with dephlogisticated air; and this combined fixed air I sometimes found to be one-third of the whole mass, though at other times not quite so much.

To ascertain this, I mixed one measure of this inflammable air from charcoal (after the uncombined fixed air had been separated from it by lime-water) with one measure of dephlogisticated air, and then fired them by the electric spark. After this I always found that the air which remained made lime-water very turbid, and the proportion in which it was now diminished, by washing in lime-water, shewed the quantity of fixed air that had been combined with the inflammable. That the fixed air is not *generated* in this process, is evident from there being no fixed air found after the explosion of dephlogisticated air and inflammable air from iron.

Notwith-



Notwithstanding the above-mentioned variations, the loss of weight in the charcoal was always much exceeded by the weight of the water expended, which was generally more than double of the charcoal; and this water was intimately combined with the air; for when I received a portion of it in mercury, no water was ever deposited from it.

The experiment which, upon the whole, gave me the most satisfaction, and the particulars of which I shall therefore recite, was the following. Expending 94 grains of perfect charcoal (by which I mean charcoal made with a very strong heat, so as to expel all fixed air from it) and 240 grains of water, I procured 840 ounce measures of air, one-fifth of which was fixed air, and of the inflammable part nearly one-third more appeared to be fixed air by decomposition.

Receiving this kind of air in a variety of experiments, but not in the preceding ones in particular (for then I could not have ascertained the quantity of it) consisting of fixed and inflammable air together, I found some variations in its specific gravity, owing, I imagine, to the different proportions of fixed air contained in it; but upon the whole, I think, that the proportion of 14 grains to 40 ounce measures is pretty near the truth, when the proportion of fixed air is about one-fifth of the whole. With respect to the weight of the inflammable air after the fixed air was separated from it, I found no great difference, and think it may be estimated at 8 grains to 30 ounce measures.

Upon these principles, the whole weight of the 840 ounce measures of air will be

that of the charcoal will be

that of the water

- 294 grains

94

- 240

334 which, considering the na-

ture of the experiment, will perhaps be thought to be tolerably near that of the air.

If the air be analyzed, the 840 ounce measures will be found to contain - 168 of uncombined fixed air = 151 grains.  
and 672 impure inflammable = 179

so that the whole 840 will weigh - - 330

Lastly, if the 672 ounce measures of impure inflammable air be decomposed, it will be found to contain

164 ounce measures of fixed air = 147.6 grs.  
and 508 inflammable - - = 30.7

so that the whole 672 will weigh - - 178.3  
which is very near to 179, the weight of the whole together.

It may, however, be safely concluded from this experiment, and indeed from every other that I made with charcoal, that there was no more pure inflammable air produced than the charcoal itself may be very well supposed to have supplied.

There is, therefore, no reason for deserting the old established hypothesis of *phlogiston* on account of these experiments, since the fact is by no means inconsistent with it. The pure inflammable air with the water necessarily contained in it would weigh no more than about 30 grains, while the loss of weight in the charcoal was 94 grains. But to this must be added the *phlogiston* contained in 392 ounce measures of fixed air, which, according to Mr. KIRWAN's proportion, will be nearly 65 grains, and this and the 30 grains will be 95 grains.

The basis to this fixed air, as well as to the inflammable, must have been furnished by the *water*; and from this it may be concluded, that the water must have been so far altered as to be changed into fixed air, which will be thought not to be any great paradox, if it be considered that, according to the

latest

latest discoveries, fixed air and water appear to consist of the same ingredients, namely dephlogistified and inflammable air. However, in this change of the water we cannot be absolutely sure that the same proportion of the ingredients is contained, and therefore it cannot be absolutely determined whether the inflammable air which it contains enters wholly into the fixed air, or not. Farther experiments, or a careful comparison of these experiments with those made by Mr. KIRWAN and others, may perhaps throw some light upon this subject. Whether the combined fixed air comes wholly from the charcoal, or whether the charcoal only supplies the phlogiston, and the water its basis, that is, the dephlogistified air, deserves to be investigated.

Before I conclude my account of the experiments with charcoal, I would observe, that there is another in which I place some dependance, in which, with the loss of 178 grains of charcoal, and 528 grains of water, I procured 1410 ounce measures of air, of which the last portion (for I did not examine the rest) contained one-sixth part of uncombined fixed air. This was made in an earthen tube glazed on the outside.

The experiments with *iron* were more satisfactory than those with charcoal, being subject to less variation; and it is still more evident from them, that the inflammable air does not come from the *water*, but only from the *iron*, as the quantity of water expended, added to the weight of the air produced, was as nearly as could be expected in experiments of this kind, found in the addition of weight gained by the iron. And though the inflammable air procured in this process is between one-third and one-half more than can be procured from iron by a solution in acids, the reason may be, that much phlogiston is retained in the solutions, and therefore much more may be expelled from iron, when pure water, without any acid, takes the place of it. I would further observe, that the produce of  
air,

air, and also the addition of weight gained by the iron, are much more easily ascertained in these experiments than the quantity of water expended in them, on account of the great length of the vessels used in the process, and the different quantities that may perhaps be retained in the worm of the tub, though I did not fail to use all the precautions that I could think of to guard against any variation on these accounts.

Of the many experiments which I made with *iron*, I shall content myself with reciting the following results. With the addition of 267 grains to a quantity of iron, and the loss of 336 grains of water, I procured 840 ounce measures of inflammable air; and with the addition of 140 grains to another quantity of iron, and the consumption of 253 grains of water, I got 420 ounce measures of air\*.

The inflammable air produced in this manner is of the lightest kind, and free from that very *offensive smell* which is generally occasioned by the rapid solution of metals in oil of vitriol, and it is extricated in as little time in this way as it is possible to do it by any mode of solution. On this account it occurred to me, that it must be by much the cheapest method that has yet been used of filling *balloons* with the lightest inflammable air. For this purpose it will be proper to make use of cast-iron cylinders of a considerable length, and about three or four inches, or perhaps more, in diameter. Though the iron tube itself will contribute to the production of air, and therefore may become unfit for the purpose in time; yet, for any

\* If the perfect accuracy of the former of these experiments may be depended on (and it may always be presumed, that those in which *little water* is expended are preferable to those in which *more* is consumed) the *water* that necessarily enters into this kind of inflammable air is about equal in weight to the *phlogiston* that is in it. But I propose to give more particular attention to this subject.

thing that I know to the contrary, the same tube may serve for a very great number of processes, and perhaps the change made in the inside surface may protect it from any farther action of the water, if the tube be of sufficient thickness; but this can only be determined by experiment.

Some estimate of what may be expected from this method of procuring inflammable air may be formed from the following observations. About twelve inches in length of a copper tube, three-fourths of an inch in diameter, filled with *iron turnings* (which are more convenient for this purpose than *iron filings*, as they do not lie so close, but admit the steam to pass through their interstices) when it was heated, and a sufficient quantity of steam passed through it, yielded thirty ounce measures of air in fifty seconds; and eighteen inches of another copper tube, an inch and a quarter in diameter, filled and treated in the same manner, gave two hundred ounce measures in one minute and twenty-five seconds; so that this larger tube gave air in proportion to its solid contents compared with the smaller; but to what extent this might be depended upon I cannot tell. However, as the heat penetrates so readily to some distance, the rate of giving air will always be in a greater proportion than that of the simple diameter of the tube.

The following experiment was made with a view to ascertain the quantity of inflammable air that may be procured in this way from any given quantity of iron. Two ounces of iron, or 960 grains, when dissolved in acids, will yield about 800 ounce measures of air; but treated in this manner it yielded 1054 ounce measures, and then the iron had gained 329 grains in weight, which is little short of one-third of the weight of the iron.

Considering

Considering how little this inflammable air weighs, *viz.* the whole 1054 ounce measures not more than 63 grains, and the difficulty of ascertaining the loss of water to so small a quantity as this, it is not possible to determine, from a process of this kind, how much water enters into the composition of the inflammable air of metals. It would be more easy to determine this circumstance with respect to the inflammable air of charcoal, especially by means of the experiment made with a burning lens *in vacuo*. In this method two grains of charcoal gave at a medium thirteen ounce measures of inflammable air, which, in the proportion of 30 ounce measures to 8 grains, will weigh 3.3 grains; so that water in the composition of this kind of inflammable air is in the proportion of 1.3 to 2, though there will be some difficulty with respect to the fixed air intimately combined with this kind of inflammable air.

Since iron gains the same addition of weight by melting in *dephlogisticated air*, and also by the addition of *water* when red-hot, and becomes, as I have already observed, in all respects the same substance, it is evident, that this *air* or *water*, as existing in the iron, is the very same thing; and this can hardly be explained but upon the supposition that water consists of two kinds of air; *viz.* inflammable and dephlogisticated. I shall endeavour to explain these processes in the following manner.

When iron is melted in dephlogisticated air, we may suppose that, though part of its phlogiston escapes, to enter into the composition of the small quantity of fixed air which is then procured, yet enough remains to form *water* with the addition of dephlogisticated air which it has imbibed, so that this *calx* of iron consists of the intimate union of the pure *earth* of iron and of *water*; and therefore when the same *calx*, thus satu-

rated with water, is exposed to heat in inflammable air, this air enters into it, destroys the attraction between the water and the earth, and revives the iron, while the water is expelled in its proper form.

Consequently, in the process with *steam*, nothing is necessary to be supposed but the entrance of the water, and the expulsion of the phlogiston belonging to the iron, no more phlogiston remaining in it than what the water brought along with it, and which is retained as a constituent part of the water, or of the new compound.

Having procured water from the scales of iron (which I must again observe is, in all respects, the same substance with iron melted in dephlogisticated air, or saturated with steam by means of heat) and having thereby converted it into perfect iron again, I did not entertain a doubt but that I should be able to produce the same effect by heating it with charcoal in a retort; and I had likewise no doubt but I should be able to extract the additional weight which the iron had gained (*viz.* one-third of the whole) in *water*. In the former of these conjectures I was right; but with respect to the latter, I was totally mistaken.

Having made the scales of iron, and also the powder of charcoal very hot, previous to the experiment, so that I was satisfied that no air could be extracted from either of them separately by any degree of heat, and having mixed them together while they were hot, I put them into an earthen retort, glazed within and without, which was quite impervious to air. This I placed in a furnace, in which I could give it a very strong heat; and connected with it proper vessels to condense and collect the water which I expected to receive in the course of the process. But, to my great surprise, not one particle  
of

of *moisture* came over, but a prodigious quantity of *air*, and the rapidity of its production astonished me; so that I had no doubt but that the weight of the air would have been equal to the loss of weight both in the scales and in the charcoal; and when I examined the air, which I repeatedly did, I found it to contain one-tenth of fixed air, and the inflammable air, which remained when the fixed air was separated from it, was of a very remarkable kind, being quite as heavy as common air. The reason of this was sufficiently apparent when it was decomposed by means of dephlogisticated air; for the greatest part of it was fixed air.

The theory of this process I imagine to be, that the phlogiston from the charcoal reviving the iron, the water with which it had been saturated, being now set loose, affected the hot charcoal as it would have done if it had been applied to it in the form of *steam* as in the preceding experiments; and therefore the air produced in these two different modes have a near resemblance to each other, each containing fixed air, both combined and uncombined, though in different proportions; and in both the cases I found these proportions subject to variations. In one process with charcoal and scales of iron, the first produce contained one-fifth of uncombined fixed air, the middle part one-tenth, and the last none at all. But in all these cases the proportion of combined fixed air varied very little.

Why *air* and not *water* should be produced in this case, as well as in the preceding, when the iron is equally revived in both, I do not pretend perfectly to understand. There is, indeed, an obvious difference in the circumstances of the two experiments; as in that with charcoal the phlogiston is found in a combined state; whereas in that of inflammable air, it is  
loose,



loose, or only united to water; and perhaps future experiments may discover the operation of this circumstance.

There is some analogy between the experiment of the calx of iron imbibing inflammable air, and the iron itself imbibing dephlogisticated air. In the former case *water* is produced, and in the latter *fixed air*. However, this case of iron imbibing dephlogisticated air more nearly resembles the case of the *blood in the lungs* imbibing the same kind of air, and in both the cases as dephlogisticated air is imbibed, fixed air is formed. This, therefore, seems to be a confirmation of the conclusion which I drew from my former experiments on blood, *viz.* that it parts with phlogiston in respiration. Only I would now add, that at the same time that it parts with phlogiston it takes in dephlogisticated air, which makes the case perfectly similar to that of the experiment with *iron*, which likewise parts with phlogiston to form fixed air, at the same time that it imbibes dephlogisticated air in contact with which it is fused.

I propose to reserve for a future communication the continuation of these experiments, containing an account of the application of the same process to other substances; but it may not be amiss just to mention a few of the *general results*, and those which have the nearest connexion with the experiments recited above.

After having transmitted steam in contact with *charcoal* and *iron* in a copper tube, I proposed to do the same with other substances containing phlogiston, and I began with *bones*, which were burnt black, and had been subjected to an intense heat, covered with sand, in an earthen retort. From three ounces of bone thus prepared, and treated as I had done the charcoal, I got 840 ounce measures of air, with the loss of 288 grains of water. The bones were by this means made perfectly white,

and had lost 110 grains of their weight. As the air ceased to come a considerable time before all the water had been transmitted through the tube containing them, I concluded that the air was formed from the phlogiston contained in the bones, and so much water as was necessary to give it the form of air.

This air differs considerably from any other kind of inflammable air, being in several respects a medium between that from charcoal and that from iron. It contains about one-fourth of its bulk of uncombined fixed air, but not quite one-tenth intimately combined with the remainder. The water that came over was blue, and pretty strongly alkaline, which must have been occasioned by the volatile alkali not having been entirely expelled from the bones in the former process, and its having in part dissolved the copper of the tube in which the experiment was made.

I subjected to the same process a variety of substances that are said not to contain phlogiston, but I was never able to procure inflammable air by means of them; which strengthens the hypothesis of the principal element in the constitution of this air having been derived from the substance supposed to contain phlogiston, and therefore that phlogiston is a real substance, capable of assuming the form of air by means of water and heat.

The experiments above-mentioned relating to iron were made with that kind which is *malleable*; but I had the same result when I made use of small nails of *cast iron*, except that these were firmly fastened together after the experiment, the surfaces of them being crystallized, and the crystals mixing with each other, so that it was with great difficulty that they could be got out of the tube after the experiment, and in general the solid parts of the nails were broken before they were separated

from

from each other. Indeed the pieces of malleable iron adhered together after the experiment, but by no means so firmly.

Cast iron annealed (by being kept red-hot in charcoal) is remarkably different from the cast iron which has not undergone that operation, especially in its being, to an extraordinary degree, more soluble in acids. With the turnings of annealed cast iron I made the following experiment. From 960 grains of this iron, and with the loss of 480 grains of water, I got 870 ounce measures of inflammable air, and transmitting steam through them a second time, I got 150 ounce measures more. The iron had then gained 246 grains in weight, and the pieces adhered firmly together; but being thin they were easily broken and got out of the tube, whereas it had required a long time, and a sharp steel instrument, to clear the tube of the cast-iron nails.

Having got water from the scales of iron and of copper saturated with dephlogisticated air, by heating them in inflammable air, it occurred to me to make the same experiment with *precipitate per se*, and I found, that the moment that the focus of the lens fell upon this substance the mercury began to revive, the inflammable air rapidly disappeared, and *water* was formed on the sides of the vessel in which the experiment was made. For want of a better fun, I could not ascertain every circumstance relating to this process; but what I did seemed to afford a sufficient proof that mercury contains phlogiston, and that it is not revived by the mere expulsion of dephlogisticated air, as M. LAVOISIER supposes; especially as *no fixed air* was found in what remained of the inflammable air. In one of these experiments 4.5 ounce measures of inflammable air had disappeared, and 1.6 ounce measure remained; and this appeared to contain some dephlogisticated air mixed with the inflammable.

Willing to try the effect of heating iron, and other substances, in all the different kinds of air, without any particular expectation, I found that iron melted more readily in *vitriolic acid* air than in dephlogisticated air, the air was diminished as rapidly, and the inside of the vessel was covered with a *black sooty matter*, which, when exposed to heat, readily sublimed in the form of a white vapour, and left the glass quite clean. The iron, after the experiment, was quite brittle, and must, I presume, be the same thing with iron that is *sulphurated*; but I did not particularly examine it. Of seven ounce measures of vitriolic acid air, in one of these experiments, not more than three-tenths of an ounce measure remained; of this two-thirds was fixed air, and the residuum of this was inflammable. I had put three of such residuums together, in order to make the experiment with the greater certainty.

Having transmitted *steam*, or the vapour of water, through a copper tube, I was willing to try the effects of *spirit of wine* through the same tube when red-hot, having before procured inflammable air by sending the same vapour through a red-hot tobacco-pipe. In this case, the vapour of the spirit of wine had no sooner entered the hot copper tube, than I was perfectly astonished at the rapid production of air. It resembled the blowing of a pair of bellows. But I had not used four ounces of the spirit of wine before I very unexpectedly found, that the tube was perforated in several places; and presently afterwards it was so far destroyed, that in attempting to remove it from the fire it actually fell in pieces. The inside was full of a black sooty matter resembling lamp-black.

Upon this I had recourse to *earthen tubes*, and found, that by melting copper and other metals in them, and transmitting the

vapour of spirit of wine in contact with them, different substances were formed according to the metals employed. The new substances hereby formed may be said to be the several metals super-saturated with phlogiston, and may perhaps not be improperly called the *charcoal of metals*.

That this appellation is not very improper, may appear from these substances yielding inflammable air very copiously when they are made red-hot, and the steam of *water* is transmitted in contact with them, just as when the charcoal of wood is treated in the same manner. The detail of these experiments I reserve for another communication, as also those of the conversion of *spirit of wine*, *æther*, and *oil*, into different kinds of inflammable air, by transmitting them, in vapour, through hot earthen tubes. In the mean time, I shall think myself happy if the communication of the preceding experiments shall give any satisfaction to the Members of the Society.

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## P O S T S C R I P T.

BEFORE I close this paper, I wish to make a few *general inferences* from the principal of the experiments above-mentioned, especially relating to the proportional quantity of phlogiston contained in *iron* and *water*.

When any quantity of iron is melted in dephlogisticated air, it imbibes the greatest part of it, and gains an addition of weight very nearly equal to that of the air imbibed. Thus the absorption of twelve ounce measures of dephlogisticated air

gave an addition of six grains to the piece of iron which had been melted in it. But there was always a quantity of *fixed air* produced in this process; and on the supposition that this air consists of the union of dephlogisticated and inflammable air, it proves that the dephlogisticated air which enters the iron expels more phlogiston than is necessary to constitute an equal weight of water, so that *water* does not contain so much phlogiston as *iron*; but the difference is not very considerable.

Admitting Mr. KIRWAN's conclusion, *viz.* that 100 cubic inches of fixed air contain 8,357 grains of phlogiston, the .13 ounce measure of fixed air, which (in an experiment recited in these papers) was found in the residuum of seven ounce measures of dephlogisticated air absorbed by iron, would not have contained more than .01 grain of phlogiston, or about .16 ounce measure of inflammable air. Then, as the absorption of 12 ounce measures of dephlogisticated air occasioned an addition of 6 grains to the weight of the iron which had absorbed it, the absorption of seven ounce measures must have occasioned the addition of 3.5 grains to the iron which had imbibed it. But the same addition of weight to iron given by *steam* (which carries its own inflammable air along with it) would have expelled near 12 ounce measures of inflammable air: consequently, about ten ounce measures of inflammable air (or the phlogiston requisite to form it) must, in the former experiment, have been retained in the iron, in order to compose the *water* which was now made by the union of the dephlogisticated air imbibed by the iron and the phlogiston contained in it: and therefore the proportion between the quantity of phlogiston in *iron* to that which is contained in an equal weight of water, may be about 12 to 10, or more accurately to 10.4.

Had no fixed air at all been found in the residuum above-mentioned, it might have been concluded, that water had con-

tained the very same proportion of phlogiston with iron. Since when iron that has been saturated with dephlogisticated air is heated in inflammable air (in which process an equal weight of water is produced, and the loss of weight in the iron is equal to that of such a quantity of dephlogisticated air as would have been one-half of the bulk of the inflammable air which disappears in that process) it might have been concluded, that one-fifth of any quantity in water had been inflammable air.

For, neglecting the difference between the weight of dephlogisticated and common air, which is not considerable, and estimating the latter  $\frac{1}{800}$ th part of water, and inflammable air at one-tenth of the weight of common air, an ounce measure of dephlogisticated air will weigh .6 grain, and two ounce measures of inflammable air will weigh .12 grain, which numbers are to each other as 5 to 1 \*.

Though, in consequence of the small quantity of fixed air which is found in the process of melting iron in dephlogisticated air, this conclusion is not accurate, it is pretty nearly so; and it is remarkable that, upon this supposition, about as much inflammable air is expelled from iron when water is com-

\* It appears from the prosecution of these experiments, that the water which is found on heating the scales of iron in inflammable air, is not formed by the dephlogisticated air expelled from them uniting with the inflammable air in the vessel, but was the water previously contained in the scales, which is made to quit its place by the introduction of the phlogiston from the inflammable air; yet that water carries out with it not much less phlogiston than was taken in by the iron, and a little more must be allowed for that water which was necessary to make inflammable air, and which could not enter the iron when it was revived; so that, on the whole, the phlogiston in the water that is found after the process must be very nearly the same quantity that is imbibed by the iron, and the water is nearly the same that would have been produced, on the supposition of its being made from dephlogisticated air expelled from the scales uniting with the inflammable air in the vessel.

bined with it, as the water itself brings along with it, as an essential ingredient in its composition. For in one experiment 296 grains added to the weight of a quantity of iron by steam, made it to yield about 1000 ounce measures of inflammable air. This would weigh 60 grains, and one-fifth of the 296 grains of water will be 59.2 grains. Again, 267 grains added to iron by steam made it to yield 840 ounce measures of inflammable air, which would weigh 50.4 grains, and one-fifth of the 267 would be 53.4 grains.

When the experiments on the melting of iron in dephlogisticated air shall be repeated on a larger scale, which it will not be difficult to do by the help of a larger burning lens than I am at present possessed of, it will be easy to reduce these calculations to a greater certainty. All that I can do at present is to approximate to such general conclusions as I have mentioned; but they are of so much consequence in philosophy, that it will certainly be well worth while to ascertain them with as much accuracy as possible. Nice calculations would be ill bestowed on the imperfect *data* which I am as yet able to furnish. Attention must also be given to the quantity of water contained in inflammable air from iron; which not being yet ascertained is not considered in these inferences. I wish only to hint in this Postscript, that some important conclusions seems to be nearly within our reach.